HUYGENS LABORATORIUM

RIJKSUNIVERSITEIT LEIDEN

Wassenaarseweg 78
Postbus 9504
2300 RA LEIDEN
telefoon 071 - 148333 tst.
telex 39058 Astro. NL.

p77. 1

Dr. Joshua Lederberg
The Rockefeller University
1230 York Avenue
New York NY 10021
U.S.A.

Uw kenmerk:
Uw brief van:
Ons kenmerk: 84055/JM/1p
Betreft:

Leiden, July 16th, 1984

Dear Dr. Lederberg,

How prescient you were! I have to admit that shortly after I started my experiments at Albany in 1970 - which also seems the distant past - your article was brought to my attention. However, as often happens I forgot about it in the subsequent years of pressing my own ideas which evolved along somewhat different lines from yours.

The point which I early emphasized was that an important key to chemical evolution in space was photochemical processing of grains by the ubiquitous ultraviolet radiation. I had come to this conclusion already some years earlier when solid H_2O was not detected in grains. Unfortunately not many people took my work seriously. For example, I could not get any grants from either NSF or NASA and when Leiden invited me to start Laboratory Astrophysics to do what I chose with a substantial initial investment in equipment and personnel it was in irresistable offer (I also happened to like Leiden). Incidentally the NSF gave me a one year grant just as I was leaving for Leiden.

A long time ago I concluded that it was impossible to understand interstellar grains unless one tried to look simultaneously at as many different aspects of the problem as possible. I have sent you a fairly complete collection of reprints and preprints not all of which you will want to read but which may give you a feeling for the way the theory of interstellar grains has evolved - from my point of view.

Curiously, although I always consider myself to be hesistant and conservative in my ideas, my work has been viewed as way out by many of my colleagues. Even the people who came to work in my laboratory were not

convinced that my grain explosion mechanism would work (either in the laboratory or in space). Incidentally you had suggested "The fission of grains might occur either by exergonic rearrangement....". Your "fission" could be interpreted as my explosion.

It is interesting for me to recall how reluctant I once was to invest my time in the possibility of a relationship between interstellar and interplanetary bodies. I think I was probably turned off by the then prevailing theories of a hot presolar nebula. However, as you can see, my own work has forced me to work on such connections. Starting with a construction of comets out of fully evolved grains one is invitably led to theories of interplanetary particles as clumps of grains which undergo various physical changes in the solar system. A number of people are looking into how aggragates of my (sic!) dust grains can evolve into the kinds of interplanetary dust particles collected by Brownlee in the earth's atmosphere.

Finally, about a year ago I started on my own approach to panspermia. My idea was that the same ultraviolet which created macromolecules from small ones could destroy biological material. My laboratory was ideally suited for this and I was fortunate to interest a fine young biochemist to work with me. We have found, as expected, that the lifetime of spores (of bacillus subtilis) is probably no more than about 2000 years in the "diffuse cloud" medium. However, if such a spore (while still viable) happened to have been collected by a passing dense molecular cloud along with other matter in the earth's upper atmosphere (a process still to be demonstrated) it could survive as long as 10^6 to 10^7 years both because of the low U.V. in the cloud as well as because it rather quickly accretes a U.V. absorbing mantle of interstellar ices.

As I reread your Science article it seems that a good deal of what I have done is to put meat on your speculations. I hope you find this satisfying.

Incidentally, if you ever see Eddie Cohen, please give him my regards. He had suggested that I could give a colloquium at Rockefeller University when I am in the neighborhood, but I never realised that there was anyone there who would really be interested. Thanks for sending me your note.

Sincerely,

J. Mayo Greenberg

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